one to each of them in turn, which we should have been quite justified in doing, having regard to their value.

Of the two volumes on the Natural History of Insects, which are published by a society of French and foreign entomologists, vol. i. is devoted to the Coleoptera, and comprises their organisation and their different orders, with a short description of each, and woodcuts showing their different stages of development. These are followed by other useful matter, and then, in the second part, we come to "Le Monde des Scarabées." The stag-beetle is here taken as an example of his family to show the anatomy of these insects. The description of their dwellings and instincts is clear, and written in such a style that it may be understood and enjoyed by those not versed in entomology. This part occupies a good portion of the book. In the pages devoted to the hunting, preparing, and keeping of beetles, beginners may find every information they require; pincers, pins, and nets are all shown, as well as the necessary requisites for the knapsack. A list of the principal entomological works is given, after which we have a lengthy classification and iconography of European coleoptera, illustrated with forty-eight plates beautifully coloured by hand.

The arrangement of volume ii., on Butterflies, is very similar to the above, and contains thirty coloured plates illustrating the butterfly, caterpillar, and chrysalis, together with the plants on which these are most frequently to be found.

"The Anatomy and Physiology of the Bee," is taken from volume vi. of the "Memorials of the Polish Society of Exact Sciences in Paris," and translated into French by M. Pillain. This work consists of twelve lithographic plates which obtained medals of merit both at the Universal Exhibition at Vienna and from the Royal and Imperial Society of Agriculture of Cracow. On these plates we have 172 figures of the various parts of a bee, greatly magnified. It is scarcely necessary to add that these are extremely well finished. In the folio we have the memoir itself, a book of forty pages, which first introduces us to the bees of different countries and the bibliography of the subject. In chapter i. the author describes the exterior parts of the bee, and in the second and third the interior and more complicated, such as the muscles, nervous system, circulation of the blood, &c. The work terminates with explanations of the figures. Altogether this is a valuable addition to an entomologist's library, and does great credit to the society from which it has emanated.

In M. Cordier's book on Fungi we have much valuable information. In the first place he treats generally of the organisation of fungi, their physiology, mode of reproduction, and geography, how to distinguish the edible from the poisonous, and he shows us how to extract this poison; he tells how this works on the animal economy and the best means of counteracting it. In the second part all the fungi useful to man are chronicled, with detailed descriptions of each order and drawings from nature.

M. Cordier has adopted Persoon's classification in preference to any other, as he takes it to be more practical; indeed he dedicates the book to his memory as the "Créateur de la Science Mycologique." The drawing and colours of the sixty chromolithographs are well worthy of note. The book also contains a glossary, table of common, and one of the scientific, names of the fungi.

The author has evidently endeavoured to make his subject as interesting and complete as possible. The style of the popular portion of the book is admirable, and *bon vivants* will be glad to be informed that there are eight pages dealing with the proper way of cooking truffles.

The two small books by H. de la Blanchère—one on the enemies of forest trees, with 162 engravings of insects and larvæ, the other on the enemies of orchards and vines similarly illustrated, form part of a large series now well known and highly appreciated in France. We have already noticed some of them, and these are in no way inferior to the former ones.

"Plants used for Food," written by A. Vogl, of Prague, translated into French by Ad. Focillon, is a practical guide for detecting the adulteration of flour, coffee, chocolate, tea, and the like.

"The Blowpipe," by E. Jannettaz, is extremely well arranged, and is a thoroughly practical guide for engineers, mineralogists, &c.; the information is accurate and condensed, and M. Jannettaz's name is a guarantee of its scientific value.

OUR BOOK SHELF

The Fifth Continent, with the Adjacent Islands; being an Account of Australia, Tasmania, and New Guinea, with Statistical Information up to the Latest Date. By C. H. Eden. With Map. (London: Society for Promoting Christian Knowledge; no date.)

THIS volume contains much information on the Australian colonies, but it is somewhat desultory and incomplete. It is not a children's book, and it will not satisfy those who are in quest of full information on the subject. It affords some idea of the history, people, and products of Australia and New Guinea, but it would be better to cut out much of what is said about the history and the people and give more space to well-digested information about the resources of the countries.

Notes by a Field Naturalist in the Western Tropics. By Henry H. Higgins, M.A. (Liverpool: Edward Howell, 1877.)

THIS is a readable record of observations made during a yacht voyage to the West Indies by Mr. Higgins, who is president of the Liverpool Naturalists' Field Club. Mr. Higgins went over well-trodden ground, and therefore we need not look for any novelties in this little volume, although much of it is interesting. The chief purpose of the voyage, undertaken by Mr. Cholmondley, the owner of the yacht, was to observe and collect tropical birds. Mr. Higgins collected, also, many specimens, both zoological and botanical, from sea and land, which are now being arranged. He may possibly, he states, publish an account of the biology of the voyage.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

PROF. FOSTER'S clear representation of what he conceives to be the effect of rarefaction reduces the question between us to a definite issue.

Having assumed that heat is flowing across an intervening layer of gas from a hotter surface A to a colder surface B, he

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says:—"Then, I imagine, the flow of heat through the gas will take place *as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as that of the contiguous solid, and whose thickness is equal (or at least proportional) to the mean length of path of the molecules." Without these layers of uniform temperature or whatever may produce an equivalent effect it follows directly from Prof. Foster's reasoning that the rate at which heat is communicated is, as I maintain it is, independent of the density, whereas *if there were* any such layers I should at once admit the force of Prof. Foster's reasoning. The whole question turns therefore on the existence of these layers of uniform temperature.

Now what evidence of such layers have we? No experimental evidence certainly; and not only has the kinetic theory not as yet been applied to explain their existence but it is easy to demonstrate that according to this theory no such layers or any equivalent can exist. For in order that the condition of heat may remain unaltered it is necessary that the rate at which heat is transmitted across all surfaces parallel to the solid surfaces which can be drawn through the gas should be the same. And the rate at which heat is transmitted is for small variations of temperature proportional to the degradation of temperature, hence if there were a layer of uniform temperature no heat could be transmitted.

It is surely incumbent on Prof. Foster in assuming the existence of these layers to give some sort of proof in support of his assumption, but not one word does he say!

I cannot allow this to pass without pointing out that the description which Mr. Stoney has given of my view is grossly wrong and is certainly not to be gathered from anything I have written. Mr. Stoney carefully turns my position. He makes out that I have explained the action in question as *arising* from *convection currents*, whereas I have from first to last maintained that it is these currents which oppose and eventually overcome the action. He makes out that my theory takes no account of molecular motion, whereas, in truth, it takes no account of anything but molecular motion, the effect of the expansion of the gas being so obviously trivial that I have never even mentioned it.

Your readers may judge of this by comparing the first of the following quotations, which is from Mr. Stoney's letter, with the others which are from my own papers, and are the only expressions, not mathematical, which I have given of my views as to action in the question:—

Mr. Stoney's.	*My Own.*
"Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented."	"Whenever heat is communicated from a hot surface to gas, the particles which impinge on the surface will rebound with a greater velocity than that with which they appropriate; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas. "And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that at which they approach.[1] "These forces arise from the communication of heat to or from the surface from or to the gas. These forces will be directly proportional to the rate at which the heat is communicated; and since this rate has been shown by Prof. Maxwell to be independent of the density of the gas, these forces will be independent of the density of the surrounding medium, and their effect will increase as the density and convection-currents diminish."[2]

The first of the quotations from my papers is followed by a mathematical expression on which I have depended for completeness, and from this expression, in which neither convection currents nor the expansion of the gas have any place whatsoever, it follows that whenever heat is steadily diffusing *into* or *through* a gas, the momentum transmitted across any surface in the direction in which the heat is diffusing will be greater than that transmitted in the opposite direction by a quantity proportional to the rate at which the heat diffuses, divided by the square root of the absolute temperature of the gas.

As to the value of what follows in Mr. Stoney's letter, I must leave it to your readers to decide. He proceeds to claim that his own theory has the advantage of being based on molecular motions, he says:—

"My explanation, on the other hand, is based on *molecular* motions which go on in the gas without causing any molar motion, and is independent of convection currents."

Then having thus attributed to me an explanation, I never even thought of offering, and having assumed the true base of my theory as alone belonging to his, he proceeds to show wherein I am wrong. And in every subsequent position which he attributes to me, he is as wrong as he is in his first statement.

Under these circumstances it would be useless for me to enter upon questions as to how far "diffusion," according to the kinetic theory may be more "sluggish" than Mr. Stoney's "penetration," or to discuss further the possibility of his "Crookes's layers."

In my last letter I showed that the condition of a gas which Mr. Stoney called a "Crookes's layer" was impossible, and I do not see that Mr. Stoney has improved his position by showing that he had arrived at the possibility of the condition by making the false assumption "*that gas is a perfect non-conductor of heat.*"

Wherein Mr. Stoney's views are at variance with the results of the laborious investigations of Maxwell, Clausius, Thomson, and others, he may best convince himself by referring to the works of these authors. Until he has read my papers and explained the discrepancies between his views and the generally-accepted laws of gases, I do not see that we have any common ground for discussion.

OSBORNE REYNOLDS

November 30

Mr. Crookes and Eva Fay

IF Mr. Wallace had read my letter in NATURE of November 29 with a little more attention, he would have seen that I did *not* refer to the *Daily Telegraph* "as an authority in a matter of scientific inquiry," but that the account I gave of Mr. Crookes's "scientific tests" was given *in Mr. C.'s own communication to the 'Spiritualist,'* which would have been reproduced without abridgment if the columns of NATURE could have admitted it.

What I hold myself pledged to show (in NATURE, if it pleases, as well as in the new edition of my Lectures) is that the "tying-down by electricity" described by Mr. Crookes in the *Spiritualist*, is no more effective in preventing the performance of juggling tricks than Eva Fay's *ordinary* tying-down under which her tricks were publicly reproduced two years ago by Messrs. Maskelyne and Cooke. And since Mr. Crookes made no mention of the *extraordinarily* sensitive galvanometer he used, which is described for the first time by Mr. Wallace in the last number of *Fraser*, I only consider myself bound to show the *method* by which, with *ordinary* apparatus, the electric test may be evaded—the trained skill of the acute *trompeuse* being very probably required to meet the more severe test now first specified.

Mr. Wallace seems to me to have been a little hasty on another point. "The supposed *exposure* of Eva Fay in America," he says, "was no exposure at all, but a clumsy imitation." As this is merely Mr. W.'s *dictum* founded upon an imperfect newspaper report, I prefer to trust the judgment of the eye-witnesses who have publicly testified to *the completeness of the exposure*. Among these are not only three of the ablest men in New York (the Rev. Dr. Bellows, Ex-Surgeon-General Mott, and Dr. Hammond), but the reporters of the very newspaper referred to which had previously shown a decided leaning to the claims of spiritualism. And their judgment is confirmed by the fact (which Mr. Wallace probably considers as a newspaper fiction, but of which I have independent testimony) that *Eva Fay was forced by the local authorities to take out a licence as a juggler* as a condition of the continuance of her public performances.

The fundamental difference between Mr. Wallace and myself as to the validity of testimony in regard to the "occult" comes out so strongly in this case that we have really no common

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[1] *Proceedings*, Royal Society, 1874, p. 407.

[2] *Phil. Mag.*, November, 1874, p. 3.

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